

The adhesion between metallic contacts consequent upon the passage of a current has been carefully investigated by Mr. Stroh, who observed it in the case of all of a great number of metals with which he experimented. My first observations on the subject (one of which is mentioned in the paper) were made with the refractory metal platinum, and not with bismuth, as the writer of the note seems to infer; and though Mr. Stroh's explanation—that the adhesion is due to fusion—is quoted, I express no opinion of my own on the matter. Whatever may be the cause, it seems evident enough that such adhesion must necessarily be detrimental to the perfect action of a microphone, though I am not aware that attention has been previously directed to this point.

It is not correct to attribute to me the opinion, as stated in the note, "that the heat generated by the current plays an important part, for in carbon this reduces the resistance, whilst in metals it increases it." On the contrary I give reasons for believing that at least a moderate degree of heat increases the resistance of loose carbon contacts. Increased current, however, is accompanied by diminished resistance, and while I am not prepared to say that heat plays no part whatever in the matter, it appears to me probable that the effect is mainly owing to some other incident of the stronger current, e.g. greater difference of potential.

My experiments on metals were not, as the writer supposes, entirely confined to bismuth. More than a hundred observations were recorded of the resistance of platinum and copper contacts under different conditions, and some of these are referred to in the paper. Owing, however, to the low specific resistance of these metals, the methods which I had applied with success in the case of carbon were found to be unsuitable, and the results obtained, though not on the whole inconsistent with those yielded by bismuth, were unsatisfactory and inconclusive. Bismuth was chosen for the bulk of the experiments, principally on account of its bad conductivity, which renders changes in the resistance of the contact easier of observation; but since it was my object to contrast the behaviour of metals with that of carbon (which is infusible), its ready fusibility is another advantage. If I had desired to make a good metallic microphone, I should probably have thought with the writer of the note that bismuth was "the very metal which ought to have been avoided." But for experiments conducted with the object of ascertaining the causes of the generally recognised fact that metals, as a class, are inferior in microphonic efficiency to carbon, it is evident that the metal which gives the poorest microphonic effects is *the very one which ought to be selected*, on account of the probability that with such a metal these causes would be most conspicuous.

As a matter of strict scientific exactness I agree with the writer that "no conclusion of any value as to metals in general can be drawn from experiments on bismuth alone." But since the physical properties with respect to which bismuth differs from carbon, and which have any probable connection with microphonic action, seem to be common in various degrees to all metallic bodies, I venture to predict with tolerable confidence, that if the experiments described in the paper are repeated with suitable apparatus, it will be found that all the conclusions arrived at with regard to bismuth (as summarised in the abstract before referred to) are also true to a greater or less extent for any other ordinary metal.

SHELFORD BIDWELL

Wandsworth, April 22

[The necessary brevity of the note to which Mr. Bidwell refers precluded lengthy quotations. At the same time it was only natural to draw attention to the weak point in Mr. Bidwell's argument, namely, that the behaviour of the metals generally could not with any certainty be argued from observations made, as Mr. Bidwell admits, on the very metal which for practical ends ought to be avoided. It is greatly to be wished that Mr. Bidwell will so far further improve the capabilities of his apparatus as not only to be able to get conclusive results with other metal, but also so as to enable him to say why his apparatus gave results that were unsatisfactory and inconclusive with good conducting metals such as platinum and copper.]

The Soaring of Birds

FOR more than twenty years I have watched with admiration the soaring of the black vulture of Jamaica (*Vultur aura*). When once well up in the air it rarely moves its wings, except to change the direction of its flight. It can soar whenever there is even a light wind.

I entirely concur with Mr. Hubert Airy in the main point of his general conclusion, as given in vol. xxvii. p. 592. "Variations in the strength and direction of current" can, as he says, be so "utilised" by birds as to enable them to soar. But a high wind is not necessary; and a downward current, even when approaching the perpendicular, may, if of sufficient velocity, be utilised.

Whenever there is a wind there will be ascending and descending currents in some places. This will be so even in a level plain which presents no considerable obstacles, such as trees or buildings, to the stream of air. The plain will be bounded by hills of varying height, and it will vary in breadth. A stream of water would merely flow more rapidly through the narrower channels; but a stream of air, being highly elastic, will also rise and fall, and it will have its eddies in planes more or less inclined to the horizon, and will often acquire a rolling motion. Assuming the existence of ascending and descending currents, the soaring is a very simple matter. *The bird contrives to remain much longer in the upward currents than in the downward.* It will glide along the ascending side of a wave of air and cut across the descending side. It will make many spiral turns in an ascending current of sufficient amplitude. I have often seen the vulture ascend thus for more than 2000 feet, keeping near a steep mountain side. If the bird encounters a descending current, of which it is instantly aware through the diminished pressure on its wings, it will either wheel to the right or left to get out of it, or, altering the pitch of its wings, will descend swiftly so as to acquire the necessary impetus for a rapid escape, or will do both.

It can also avail itself of inequalities in the velocity of horizontal currents flowing parallel to one another at the same elevation. The bird, let us suppose, encounters a strong horizontal current, as warm as it is rapid, issuing from a mountain valley or a cutting through a forest. Instantly throwing its wings into a plane nearly vertical, it receives on them the force of the current, and in a few seconds acquires its velocity. Pitching its wings also for a downward flight it shoots quickly through the current, having acquired a speed more than sufficient for the recovery of its original elevation. If the current be very strong and very narrow, it need not be horizontal, but may approach the perpendicular. The bird will not remain in it long enough to be carried far down, while it acquires an impetus much more than compensating for the slight loss of elevation. It must be remembered that when the bird is gliding at a high rate of speed, the resistance of the air, through its inertia, to any movement except in the plane of the wings, almost equals that of a solid body, and a change of direction causes a very slight loss of momentum.

What rapidity of currents is necessary for soaring must depend in great measure on the structure of the bird. The vulture is, I believe, comparatively heavy, but I think that, having once acquired speed by a short and steep descent, it can glide through still air (or at right angles through air having a uniform horizontal notion) at the rate of twenty miles an hour, descending not more than one in twenty. If, therefore, the bird could be always in an upward current of only one mile an hour, it could maintain itself in the air. A gentle breeze of ten miles an hour, with one mile an hour of ascent—and a much steeper ascent than this must be frequent enough where there are hills—would suffice to sustain the bird; and as an average of ten miles an hour implies local or occasional gusts of greater velocity, of which the bird knows how to avail itself, it could ascend in such a current, and so be able to work to windward. If besides hills of moderate inclination, there are also trees, walls, houses, the air will often be driven upward, vertically or nearly so, with as great or even greater speed than that of its average horizontal movement; and of this upward movement the birds avail themselves most skilfully. I have frequently seen the vultures working their way thus against a high wind. Their movements are very irregular. Sometimes, to avoid a violent gust, they will drop almost perpendicularly to within a yard or two of the ground, and shooting abruptly sideways with the high velocity gained by the drop, will get into an upward current in which, if ample enough, they will wheel, or else will cross and recross it, till they have gained a sufficient elevation, and then, taking advantage of a lull, will glide to windward.

With a breeze of only five miles an hour, there will be in many places upward currents of high inclination caused by the usual irregularities of surface. Keeping sometimes in these and sometimes in currents more slightly ascending, for, say, two-

thirds of its time, and utilising also, as I have above explained, the more rapid of the descending currents, the bird can more than sustain itself. It can at will glide to windward at the rate of fifteen miles an hour against the breeze, losing of elevation only one in twenty.

R. COURTEENAY

L'Ermitage, Hyères (Var), France, April 28

Flight of Crows

I CAN corroborate the observation of Mr. Murphy as to the oblique flight of crows. When I have seen them so flying there has always been a cross current, and they have merely kept their heads a little to the wind.

Cambridge

THOS. MCKENNY HUGHES

Sheet Lightning

Du choc des opinions jaillit la vérité. I still adhere to your assertion that sheet lightning is not, at least in most cases, the mere reflection of a common but distant storm. On the highlands of Ethiopia, in the years 1842 to 1848 I was diligently engaged in investigating the electrical phenomena so frequent in that region. The details of my observations were printed in 1858 by the French Institute, and I have published again my results in my "Observations relatives à la Physique du Globe" (Paris, 1873). The following cases may be of interest:—

Near the zenith eight successive flashes of lightning were seen 21 seconds before their thunder, which lasted exactly 12 seconds. Another day it lasted 24·4s. thirty successive times, and, as previously, without any rain. My greatest observed interval was 111·2s., corresponding to a distance of 38,500 metres, &c.

I have seen more than once straight or zigzag lightning unaccompanied by thunder. One afternoon it went to and fro twice between two horizontal cloud banks, and ended in sheet lightning which illuminated, not the lower dark bank, but only the under surface of the upper cloud. I have observed frequently thunder without lightning and lightning without thunder.

When in Adwa I recorded silent sheet lightning towards Gondar, 240 kilometres distant, where a violent storm was raging at the same time. Before leaping to a hasty conclusion, let us hear a case bearing pointedly to the opposite opinion: in 1845, at Saga (latitude 8° 11'), a semi-transparent fog which had mantled over the valley, and could not be more than 3500 metres distant, gave out a flash of sheet lightning without thunder.

Although my numerous observations have given me a strong bias in favour of your opinion, I do not wish to impose it on reluctant philosophers, but suggest the following system to clear up the question:—Let two observers, A and B, 40 or 50 miles asunder, mention instances of lightning seen in each other's true bearing. If they can also secure the help of a third observer located on or near the straight line from A to B, and who can watch in two opposite directions, many important results may be obtained.

ANTOINE D'ABBADIE

Paris, May 5

The American Trotting-Horse

MR. BREWER'S memoir on the evolution of the breed of the American trotting-horse (NATURE, vol. xxvii. p. 609), and the statistical tables that accompany it, are full of interest, but I only propose now to concern myself with the latter, which may be easily and usefully discussed by employing a statistical method that I have long advocated. In explanation I will begin by extracting the final terms of four of the lines of his table, as follows:—

Year.	2.27 or better.	2.25 or better.	2.23 or better.	2.21 or better.	2.19 or better.	2.17 or better.	2.15 or better.	2.13 or better.	2.11 or better.
1871	99	40	17	12	6	1			
1874		98	40	16	11	5	1		
1877			105	51	19	8	2		
1880				106	41	14	6	2	1

The meaning of these entries are, that in the year 1871 there were 99 horses that could trot a mile in 2 minutes 27 seconds, or less; that in the same year there were 40 that could trot it in 2 minutes 25 seconds, or less; and so on. Their significance is

that the rate per mile of the hundred fastest American trotting-horses has become 2 seconds faster in each successive period of 3 years, beginning with 1871, and ending with 1880; also that the relative speed of the hundred fastest horses in each year is closely the same, though their absolute speed differs.

We may read the table in another way. If the number of horses that can run a mile in 2 minutes 27 seconds or less is 99, we may infer without risk of sensible error that the 99th horse in the order of running accomplishes a mile in *that time exactly*, because the 100th horse certainly takes a longer time, and it is statistically incredible that the rate of the 99th and of the 100th horses should differ by more than a barely perceptible interval. For the same reason we may infer that the 40th horse in that same year runs a mile in 2 minutes 25 seconds, and so on. We can now draw curves, and by graphical interpolation find with the greatest facility the mile rate of the horse in *any* order of running in any year that we please to select. I have selected the 100th, 50th, 20th, and 10th horse respectively for each year beginning with 1874, when we are informed that the returns first begin to be accurate, and have thrown the results into the following simple table. The curves obviously required a little smoothing here and there, and in three or four places the readings have been thereby modified by one or two tenths of a second. Otherwise they are given directly from the rough plottings.

Number of Seconds and Tents of Seconds in Excess of Two Minutes that are required for Running One Mile by the Horses whose Order in the Rate of Running in each Year is given at the Top of the Columns

Year.	100th.	50th.	20th.	10th.
1874	25·1	23·4	20·5	18·8
1875	24·1	22·5	19·9	18·2
1876	23·5	21·6	19·5	17·7
1877	22·9	21·0	19·0	17·4
1878	22·1	20·2	18·5	17·0
1879	21·3	19·6	18·0	16·6
1880	20·8	19·3	17·6	16·0
1881	20·4	18·8	17·2	15·7
1882	19·9	18·4	17·0	15·4
Anticipated } 1890	16·8	15·5	14·4	13·4

Mem.—The first horse runs the mile in about 5 or 6 seconds less than the tenth horse.

It will be found on plotting the figures in the vertical columns into curves, that they run with much regularity and differ little from straight lines. The general conclusion to be derived from them is that the improvement of the running shows as yet little tendency to slacken, though no doubt if the number of horses bred for trotting ceased to increase yearly at the same large rate as hitherto, it might do so. Supposing, however, the conditions to be maintained, I should anticipate that in 1890 there will be about 15 horses that will run a mile in 2 minutes 15 seconds or less, and that the fastest horse of that year will run a mile in about 2 minutes 8 seconds.

FRANCIS GALTON

The Shapes of Leaves

MR. GRANT ALLEN'S papers in NATURE will evidently serve to direct attention to a most interesting subject which hitherto appears to have been much neglected. Every contribution of observed facts may tend to throw further light upon it, and I therefore venture to remark that one cause of the frequently filiform character of the leaves of water-plants appears to be the elongating action exercised upon the cells by the pressure of a rapid current of water, since it is obvious that growth must take place in the direction of the least resistance. With a radiate-veined leaf the tendency must be towards lateral pressure, which would compress and elongate, and so give a linear form to the leaf-cells. I have been much interested to observe that on the seashore, in places where Fuci are exposed to this action by the ebbing tide, as when growing on the edge of a large boulder or hanging over its sides, the fronds and even the receptacles become unusually elongated.